United States Department of the Interior

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To: John Stein, Ph.D.
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From: Howard Schaller, Ph.D.
Project Leader

Re: USFWS Columbia River Fisheries Program Office Comments on NOAA Fisheries technical Memorandum– Effects of the Federal Columbia River Power System on Salmon Populations

The Columbia River Fisheries Program Office of the U.S. Fish and Wildlife Service has reviewed the NOAA Fisheries (NOAAF) Draft Technical Memorandum of December 21, 2003 titled 'Effects of the Federal Columbia River Power System (FCRPS) on Salmon Populations'. We have provided the following technical comments to assist you in finalizing this paper. As described in the memorandum from NOAAF to the Columbia Basin Co-mangers, NOAAF is in the process of revising the 2000 FCRPS Biological Opinion (Biop). The draft technical memoranda were crafted to provide the most current scientific information on the impacts of the FRCPS on ESA-listed salmon in the Columbia River Basin. This Memorandum identifies that determining the extent to which direct and indirect factors of the FCRPS negatively affect salmon populations is critical for identifying measures to assure salmon survival. NOAAF's recognition that the impact of indirect affects of the hydrosystem on salmon survival is also key for identifying additional FCRPS measures, is a concept supported by many of the past studies (Kareiva et al. 2000, Deriso et al 2001, Peters and Marmorek 2001, Budy et al. 2002, Wilson 2003). We believe given this weight of evidence on the importance of indirect mortality, that addressing the impact of indirect mortality from the FCRPS is critical information needed for the Biop revisions. The memorandum recognizes that in order to estimate the extent to which the FCRPS (and the sum of all other human induced activities) affects salmon populations requires trying to tease out an understanding of direct and indirect impacts in concert with natural variability in salmon populations.

The major concern we have with this memorandum is that it does not present an organized scientific approach for identifying and assessing indirect (delayed) mortality of the FCRPS as it relates to recovering listed salmon populations. This technical paper recognizes the PATH framework, which presented an analytical framework for estimating delayed mortality (Deriso et al 2001) and evaluating the affect of alternative hypotheses for delayed mortality. However, the document makes no attempt at incorporating any of its new hypotheses and testing them in a rigorous manner with the additional information presented throughout the document.

Overall, the document suffers from the lack of any decision framework guiding the presentation and interpretation of analysis and evidence of FCRPS impacts on salmon and steelhead. Burden of proof is applied inconsistently among the various hypotheses. Insufficient attention is devoted to detailing alternative hypotheses for the cause of particular impacts, e.g. stress and disease related reasons for low post-Bonneville survival of transported fish. Inappropriate metrics are sometimes used. Historical information is applied selectively, sometimes leading to inaccurate statements or misleading graphics. Much relevant literature, published either in the peer-reviewed journals or as technical documents of involved agencies, is omitted (some are referenced at the end of these comments). Scant attention is given to several of the impacts from construction and operation of the FCRPS on the physical, and hence the ecological, environment of Columbia and Snake Rivers. An example is the effect of impoundment and water management on water temperature [for a review of temperature effects, see McCullough (1999)].

In summary, the document is poorly organized. There are no clear problem statements and the methods, results, and discussion do not logically flow from one another. We could not consistently trace a problem statement to a method and then to the corresponding result and discussion. Then finally, it is difficult to review a document that does not contain a summary or conclusion section.

The staff of the Columbia River Fisheries Program office will be willing to assist NOAAF scientists with data and analysis need to finalize these documents. If you have any questions you can contact me or Paul Wilson by phone or email: Howard_Schaller@r1.fws.gov. See the specific comments below.

Concerns relating to specific text, tables, and figures:

Pgs. 4-5, Description of delayed mortality in The PATH process section. The term here that is wanted is "extra mortality", as described in Peters and Marmorek (2001). "Delayed mortality" refers to mortality attributable to the hydrosystem, as defined correctly later in the document (pg. 21).

Pg. 5, definition of "Regime Shift" hypothesis. The definition omits a key assumption of the Regime Shift hypothesis of PATH, namely that "these climatic effects on Snake River stocks are systematically different from effects on lower Columbia River stocks" (Peters and Marmorek 2001).

Pg 5 'Subsequent to PATH Levin and Tolimieri (2001) and Levin (2003) found that Chinook salmon populations used in PATH life cycle models… had different productivity, and varied between time periods, but not consistently with change in ocean conditions.' We have previously reviewed Levin and Tolimieri (2003) and identified that a major problem with their approach appears to be that the Ricker functions were improperly fit to the spawner and recruit data (DeHart 2003). These short comings include; not accounting for clear non-stationary process in the time series of data, aggregating individual populations into a single index, improperly fitted parent to progeny functions, results are sensitive to selection of time series, and poor documentation of methods.

Pg. 5-6, Evaluations of stocks subsequent to PATH. This section omits mention of evaluation of Snake River spring-summer chinook stocks other than Kareiva et al (2001). An analysis that evaluated alternative recovery strategies more comprehensively was Wilson (2003). The statement that past improvement at the dams resulted in little potential for productivity improvement in the migration corridor is not accurate. As Wilson (2003) notes, the low sensitivity of population growth rate to downstream in-river survival rate is due to the strategy of transporting a high proportion of migrating smolts. The low potential of in-river survival to increase productivity applies to spring-summer chinook but not necessarily to other species. For example, the in-river survival rates of summer-migrating fall chinook are so low, and transport proportion lower because of poorer guidance, that improved in-river survival may materially improve productivity.

P 7-8. SARs for untagged Snake River spring/summer Chinook population
The methods used to estimate SARS for the untagged population is poorly documented. NOAFF needs to provide the details on the methods used to determine the proportion of the adipose clipped and unclipped hatchery adults and smolts. This information can strongly influence the estimate of SARs due to the supplementation hatchery fish which have marks besides adipose clips. The estimates of hatchery and wild adults to the dams should be coordinated with the efforts of the Columbia River Technical Advisory Committee (TAC).

Pg. 8, Population trends for other unmarked stocks. Good return rate data for other wild spring and summer chinook are available, e.g. Yakima River (Fast et al. 2001). Comparing the 2001-2003 to a longer time frame is potentially misleading. The recent period does not constitute even one salmon generation, and is so short the median is equivalent to the returns of the second highest year. Comparing returns from the second highest return year to a longer term average leads to the unsurprising conclusion that a very high year is higher than an average year. Return rates have been very high in the past, after the completion of the FCRPS (mid 1980s); predictions made about the future at that time would have been much more optimistic than reality proved to be. Further, a longer "baseline" time series could be used for comparison, and would better place the improvement in the last 3 years in context.

Pg. 17, Transportation evaluation of yearling migrants. Fish PIT-tagged at Lower Granite Dam are not true control fish—they do not reflect the experience of untagged fish that are not transported. In fact comparing results from Table 15 and 19, and Tables 16 and 20 indicate lower survival rates for fish marked at Lower Granite dam than those marked above.

Pg. 19, Non-PATH derived *D.* Estimating within-season T:Is and *D*s is potentially useful, but it is unnecessary and inappropriate to use these temporal *D* values for an annual estimate of *D* for the entire population. This is because the within-season *D* estimate requires use of fish collected and tagged at LGR dam, and they may experience different survival rates to adulthood than untagged non-transported fish (see CSS annual reports- Berggren et al. 2003, and NOAAF tables 15 and 19,16 and 20).

Pg. 20 Size Selectivity   The document refers to information from Zabel et al. (In Review) that they claim suggests that the lower return rates for multiple bypassed fish may be explained by size selectivity of bypass systems. This information is used to discount all the previous peer-review publications concluding that collecting and bypassing in-river juveniles reduces their probability of returning as adults. The methods and information for Zabel et al. need to be presented in order to evaluate it's merit, since the publication is in review.

Pg. 21, Delayed mortality.   The term "delayed mortality" is defined correctly here, unlike earlier. It's unclear why at least 5 adults are necessary to estimate SAR confidence intervals. Confidence intervals can be generated from profile likelihood methods, if at least 1 adult returns (e.g. see 2002 CSS Final Annual Report, Chapter 4). The null hypothesis test set up here is inappropriate to the context. It poses a strict burden to prove that an action is having a negative effect on listed species. If this kind of burden is applied universally, threatened and endangered species would get little protection, given the inherent variability in data from natural systems. No analysis of power is proposed or performed here, to gauge the probability that a significant difference exists but wasn't found by the experiment.   Steidl and Thomas (2001) note that others have suggested that Type II errors be considered paramount when monitoring endangered species; or at least that Type I and Type II errors be balanced based on their relative costs. Shrader-Frechette and McCoy (1992) give reasons why in applied cases, Type I error is often more acceptable than Type II error, whether the null hypothesis is "positive" (no harm) or "negative" (no benefit).   Type II error leads to possible harm or loss of benefit, respectively. In endangered species recovery activities, if a Type II error is committed, a population could be on its way to extinction before the decline is detected and preventative action is taken. Conversely, if the population is monitored after initiating recovery actions, and the population is actually increasing, a Type II error would lead to the mistaken inference that the actions are not having the desired effect, perhaps jeopardizing continuance of those actions. Even better than a reliance on traditional significance testing, a structured, formal, probabilistic decision process would be most useful here and in other hypotheses testing situations.

Pg 22. Trends in Populations. The SARs presented in figure 5 appears inconsistent with previous estimates, where the median estimate was 4.3% with a range of 3.7%-7.3% (Marmorek et al. 1998). NOAAF did not document why these historic SAR estimates differed from past estimates.

Pg 22. SARs for ESUs other than Snake River spring-summer Chinook. There are SARs available for Yakima River spring Chinook starting in brood year 1983 (Fast et al. 2001) and John Day River spring Chinook smolt year 2000(ODFW-Ron Boyce).

Pg 23. Travel time. The annual median travel time, with the exception of the 2001 low flow year, varied by only few days. The fact is that the BIOP spring flow objectives were meet or closely approximated and provided little variation in mainstem velocities.

Pg 26. The document identifies that hatchery and wild chinook and steelhead have similar juvenile survival rates to hatchery fish. However, the authors later identify that smaller fish do not survive to return as adults at the same rate as larger fish. The majority of wild fish appear smaller than their hatchery counterparts. From these sets of information there seems to be an inconsistency between juvenile survival rate patterns and those for adult return rates.

Pg. 29, Snake River subyearling chinook salmon. Smith (2003) is cited, but not included in references.

Pp. 36-38, Relationships among flow, temperature, travel time and survival. Smith et al. (2002) failed to look at the power to detect a relationship between survival and river conditions. See Appendix C of USFWS comments on Mainstem Flow Amendments, Feb. 2003. In addition, the document should reference the SFTAFM (2003) analyses and results. These methods established cohorts (minimum cohort size and coefficient of variation criteria) that differed from those used by NOAAF, and showed the juvenile survival rates were significantly related to water travel times for both yearling chinook and steelhead. In this analysis SFTAFM (2003) concluded that juvenile migration conditions and ocean climate conditions were both influential in explaining patterns of adult recruitment of Snake River spring and summer chinook (spawner to spawner ratio) and SARs in Snake River spring and summer chinook and steelhead.

Pg. 36, Snake River subyearling chinook salmon. Anderson (2003), cited here, is not included in reference list.

Pg. 37 -45 Annual SARs for Spring Migrant Fish. The discussion and tables in this section are confusing when describing the non-detect category of fish (e.g. Table 13 and Table 17, Table 15 and Table 19). The fish in Table 13 were not detected at a mainstem dam and had higher survival than the fish in Table 17 that were detected and marked at Lower Granite dam. The results for steelhead were similar (Table 14 and 18). The true non-detect category (Table 13 and 14) would best represent the conditions that the run at large experienced while on their downstream migration. These true non-detect categories should be used for comparing survival rate trends of the various treatment groups (transport and inriver) and estimating D values.

Pg. 45, Annual estimates of differential post-Bonneville Dam survival (D). It is an overstatement to say that all management decisions must consider temporal variations within seasons in SARs. Some decisions will inevitably draw on long-term means and distributions of key parameters like annual SARs and ratios of annual SARs. These annual values represent the success of a year’s migration under uncontrolled seasonal variation.

However, an important finding is that there appears to be delayed mortality of transported smolts as reported by D values being substantially lower than 1. In addition, the values appear much lower than the 0.7 value used in the matrix model evaluation of hydrosystem alternatives

(Kareiva et al. 2000). The temporal values of D are from information using fish that had been detected once. This information may be useful in a relative sense, but the bottom line is annual D values appear low and would be consistent with maintaining a spread the risk management approach by transporting and keeping fish inriver with good flow and spill conditions.

Pg. 46, (1). An unweighted, mean estimate of D is not a good representation of the available information on D. CSS is addressing this and will be providing appropriate estimates of central tendencies and distributions of annual D values.

Pg. 46, (3). The first sentence is a confusing statement—it's unclear whether what is meant is that $D \approx S_i$ or that $D \approx 1$. Please clarify.

Pg. 46, (5). CSS is preparing manuscripts documenting work that allows useful information to be derived from multiple years of data on D, some with very wide confidence intervals.

Pgs. 47-50, Tables 21-24. How are the confidence intervals reported here determined? Are these just the ranges of D estimates from individual sites? How is the weighting done? Without this information, it's impossible to determine the usefulness of these ranges. CSS is preparing appropriate estimates of confidence intervals on D.

Pg. 50, Temporal SARs for spring migrant fish. It is inaccurate to say that annual T/Is should not be used as the basis for management decisions. It depends on the nature of the decision, as noted earlier regarding annual *D* values. For instance, NOAA Fisheries has published annual estimates of in-river survival and encouraged their use in management, even though in-river survival is variable over the season.

P. 51, Temporal D within season. The temporal D estimates are only useful in a relative sense. In-river migrants collected and tagged at Lower Granite dam reported lower SARs, tending to overestimate the temporal D values.

Page 52. Figure 26. These graphs seem unfinished, with labels missing. It's impossible to relate the figure to the caption with respect to which lines represent which species or migration path.

Pg 53 Discussion- Snake River spring-summer chinook. The document concludes that PIT tagged fish do not provide good information on absolute return rates of adult fish. This was based on comparing PIT tagged SARs to their estimates for SARs for the untagged population. However, other evaluations (Keifer et al. 2002 and updated Keifer 2004, personal communication) did not show a lower SAR for PIT tagged wild spring chinook compared to the untagged population (Figure 1). We believe NOAAF should reconcile estimates of wild and adult fish returning to the dams with TAC estimates.

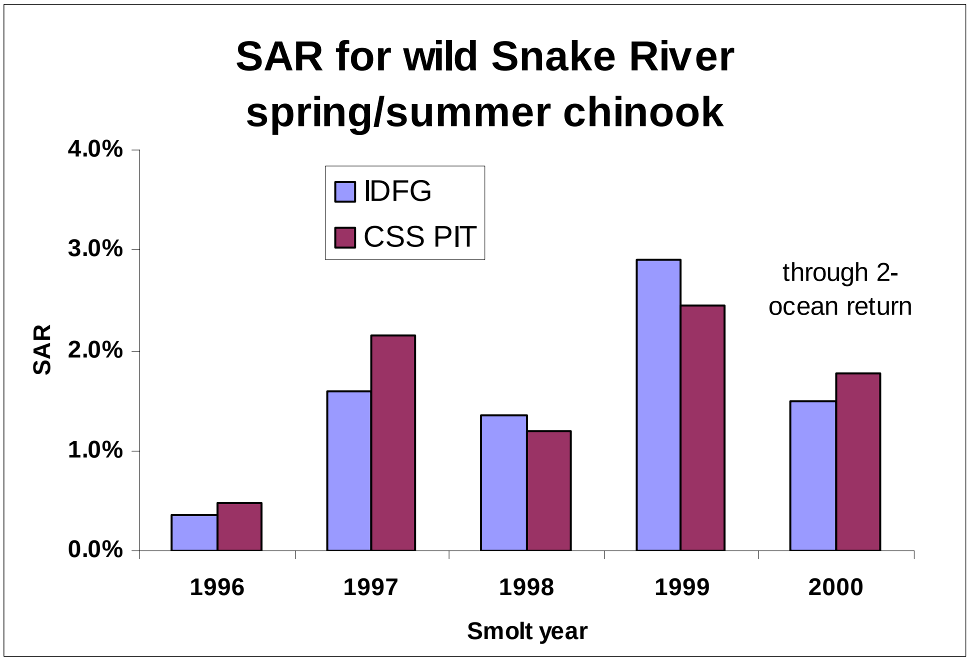

Figure 1. Estimated SARs for wild Snake River spring/summer chinook, for the untagged fish (IDFG) and for PIT tagged smolts from the CSS. (Keifer, personal communication).

Pg. 54, first paragraph and Figure 28. The statement that SRSSC stocks currently have survival as high or higher than they had in the 1960s doesn't indicate whether the much greater harvest rates in the 1960s are accounted for. Figure 28 suggests that they haven't been, since escapement alone is included. The figure is misleading because recent returns are subsidized in large part by very low harvest rates, removing much of the contrast with historical return rates, as noted by Wilson (2003). Again, the estimates in this document for the 1960s does not match with previous estimates of SARs ranging from 3.7% - 7.3% (Marmorek et al. 1998).

Pg. 55, second paragraph. The document gives one plausible hypotheses for delayed mortality of transported fish, while ignoring several others which are inherent in transportation and can't be fixed by altering the timing (such as stress, disease, lack of selection in the barge, etc.)

Pg. 56, second paragraph. The claim that delayed mortality doesn't exist because of temporal variability in SARs and *D*s relies on flawed reasoning. A seasonal trend in survival rate due to underlying natural variation or the interaction of natural variation with anthropogenic factors doesn't mean that human activities aren't responsible for any mortality. This holds for SARs as well as for reach survival rates. The figure below shows how a factor within management control, e.g. flow, can affect survival even though uncontrolled factors vary within a season or between years.

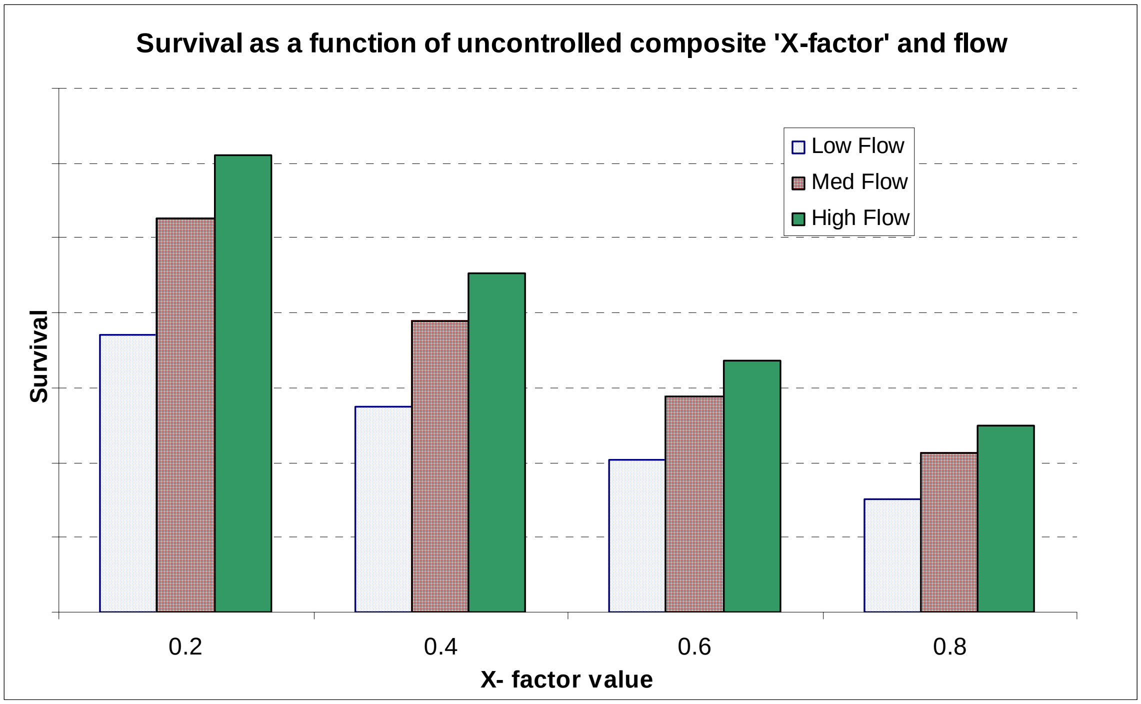

Figure 2.

The authors' state that 'the hypothesis that transportation induced stress (Budy et al. 2002) causes lower adult returns is not supported by the temporal variability in measured values of D and SARs.' The hypothesis that survival is influenced by compromised fish condition due to the hydrosystem in Budy et al. does not mean that SARs would be constant across the season. The cumulative affect of stress events will increase the vulnerability of fish to mortality factors, for both constant and fluctuating conditions over a season.

Pg. 57, first paragraph. Power to show differences in expected return rates of fish with different juvenile migration histories can be increased through application of appropriate, e.g. Bayesian, methods.

Pg 57. Effects of changing ocean conditions. This section supports an approach that incorporates the affect of ocean conditions when assessing the impacts of hydrosystem configuration and operations. This could be accomplished by implementing an analytical framework for assessing salmon populations similar to Deriso et al. (2001).

Pgs. 60-61, Diversity. The document should site Isaak et al. (2003), who addressed this issue. They emphasized that efforts should be made to desynchronize populations of vulnerable species that have become strongly correlated. They also note that the FCRPS, and attempts to artificially optimize migration timing every year may be obstacles to doing this: "Recent data suggest that the timing of smolt migration through the hydrosystem strongly affects juvenile survival and adult returns (J. Williams, unpublished data). If temporal patterns in smolt migrations and hydrosystem survival are consistent among years, the hydrosystem could effectively act as a filter that selects for particular life histories, ultimately decreasing the range of viable life-history options."

Pg. 62, first paragraph. The statement that measures of annual D have little value is inaccurate and should be deleted, for reasons noted earlier.

Pg. 64. Difficult to review this document without a conclusion section.

References

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